THURSDAY, MAY 13, 1875

LORD HARTISMERE'S VIVISECTION BILL

THE Bill brought forward in the Upper House by Lord Hartismere for regulating the practice of Vivisection deserves special attention on account of its being the first important legislative attempt to restrict the prosecution of physiological research.

It enacts that it shall not be lawful for anyone to perform a vivisection except in a place which is registered in pursuance of the proposed Act, the registration being in such form and under the management of such persons as the Secretary of State shall appoint. The registration certificate is to be renewed once a year; it may be cancelled at any time on its being proved that any provision of the Act has been contravened, and the place registered may be visited at any time by any inspector of anatomy. Complete anæsthesia is compulsory, and curare is not to be deemed to be an anæsthetic. The Secretary of State may grant special licenses for the performance of vivisections in which anæsthetics are not employed; there shall be paid in respect of every such license a sum not exceeding ten pounds, and each license is to continue in force for six months.

In the framing of this Bill there is a serious misrepresentation of the true requirements of the case. The source of error lies in the fact that it is taken for granted that there is only a single class of physiological workers. Such, however, is not the case; there are two distinct classes, and although we agree with the tenor of the Bill as far as one class is concerned, we are certain that it would so severely affect the other that its results would be seriously detrimental to the prosecution of physiological research in this country.

Among ourselves there are several scientific men who devote part of their life to the study of the problems of the vital mechanism. Some do so from the inherent interest of the subject; others from a desire to obtain a further insight into pathology and disease generally. In the course of their investigations it is now and then absolutely essential for the completion of a line of argument, or for the acquisition of the knowledge of the collateral phenomena attending some previously recorded result, that an experiment or experiments should be performed on a living animal. Those whose mental development leads them to conduct investigations of this character are frequently peculiarly unwilling to do so in public institutions. It is their spare minutes, when they are entirely their own masters, that they employ in their favourite study. Are they to be compelled, against their natural dispositions, either to obtain an official license for the performance of these experiments on their own premises, or, as an alternative, conduct them in some previously specially licensed establishment which is under the control of others? The necessity for such a method of procedure would deter many an excellent worker from commencing investigations which he recognises to be so much impeded by *legal* restrictions. There might as well be a tax on astronomers directing their telescopes to any special planet or to the moon. The public may feel certain that students of the class to which we refer will never go beyond the limits of the innate laws of sympathy present in all civilised humanity. Such do the most valuable work in a scientific point of view; and any legislative measure which in any way affects them injuriously, either by rendering the whole research apparently too formidable at the outset, or by the introduction of unpleasant details during its prosecution, ought most strenuously to be resisted. The power of turning to a practical end the results of inductive reasoning is the basis of the British nature. Inductive research cannot be had for money; it is always a labour of love; it is not fair to put impediments in the way of it.

The class of physiologists to whom legislative restrictions with regard to vivisection do apply, is the teachers. There is no doubt that those who assert that the performance of vivisectional demonstration is unnecessary will have the sympathy of the majority. A fact may be learned from books or by practical demonstration. As far as natural science goes, the extra time which has to be expended in obtaining the results practically is generally quite made up for by the accessory details introduced, which are many of them omitted in written or verbal descriptions. Observation is a far more sound basis on which to start fresh work than the knowledge acquired from books alone. The student should therefore, where nothing counter indicates, have the opportunity of repeating, on his own account, the experiments he reads of. In the case of practical physiology, however, another consideration has to be introduced. Here the subjects of experiment are sentient beings, and the question comes to be whether the advantages of the practical verification of fully described phenomena which involve pain are counterbalanced by the injustice done in the production of the pain itself. We think not, and are therefore fully in favour of legislative restrictions on the powers of those who wish to employ living animals for the purpose of demonstration, even where anæsthetics are employed, because there is a tendency among those who are in the habit of repeating experiments to neglect those parts of them which are not absolutely necessary. But any measure which in any way impedes original work, as does the Bill before us, ought, in our opinion, to be strongly opposed.

GEIKIE'S "LIFE OF MURCHISON"*

II.

Life of Sir Roderick I. Murchison, Bart., F.R.S. etc. Based on his Journals and Letters. With Notices of his Scientific Contemporaries and a Sketch of the Rise and Growth of Palæozoic Geology in Britain. By Archibald Geikie, LL.D., F.R.S., Director of H.M. Geological Survey of Scotland, and Murchison Professor of Geology and Mineralogy in the University of Edinburgh. 2 vols. Illustrated with Portraits and Woodcuts. (London: John Murray, 1875.)

MR. MALLET, in a memoir published in the *Philosophical Transactions* (vol. 163, p. 147), which has attracted attention as much for the boldness of its tone as for anything else, has laid down the dictum that no sound progress can be made in geology unless the investigator be also mathematician, chemist, and physicist. Now, Murchison was none of these, yet he would be a

* Continued from p. 3.

bolder man than the writer of that memoir who should affirm that no sound progress was made in geology by him.

It is true enough, no doubt, as Prof. Geikie says, that "he was not gifted with the philosophic spirit which evolves broad laws and principles in science," and he therefore contributed nothing to this branch of geology. It is strange, in fact, that when he did express any opinion on debated theories—and he did so frequently with vehemence—he generally took that side which the advance of science has condemned as untenable; so that the only assistance he gave to theoretical geology was that of affording the holders of any new theory the notorious advantage of having some one to argue against. He made no speculations himself, but only discussed those of others. In fact, "he had the shrewdness to know wherein his strength lay. Hence he seldom ventured beyond the domain of fact, where his first successes were won, and in which throughout his long life he worked so hard and so well. In that domain he had few equals."

But for the observation of geological facts there is no necessity for a universal acquaintance with science, however great an advantage such an acquaintance may be; and this is proved by the successful labours of many a field geologist—by the example of Wm. Smith, so often called the Father of English Geology, who had no such advantages, and by Murchison himself, as these pages of Prof. Geikie abundantly show.

Yet there are qualities requisite for such work as Murchison's, which are rarely so abundantly possessed as by him; they are, a keen perception of the really essential features of a district, or, as Smith somewhat quaintly expressed it, "a fine eye for a country;" a power of correlating apparently dissimilar objects; and last, not least, an untiring industry and perseverance that persist in pursuing an intricate subject until it is fully mastered. These appear in all his work, and are well brought out in his "Life."

Although the name of Murchison is now indissolubly connected with Palæozoic rocks, he did not begin his geological work among them, but among those easier Secondary rocks in which the order and arrangement is so much clearer. His first work, in 1825, was a "Geological Sketch of the North-western extremity of Sussex and the adjoining parts of Hants and Surrey," which was certainly up to the average geology of the time, and gave promise of better things in the future. Indeed, when it was thus seen that he had the ability, and intended to be a worker in the science, he was elected to the secretaryship of the Geological and fellowship of the Royal Society, rather from the hope of what he would do than from what he had done—and fortunately the hope was not disappointed.

His next work was the determination of the age of the coal-beds of Brora on the east coast of Scotland, in connection with which he described those remarkable remains of Secondary rocks so marvellously preserved on both sides of Scotland, and which have lately been the subject of such admirable and beautiful memoirs by Judd and others.

The difficulties he found in understanding some of the rocks he saw on this tour induced him to seek the co-operation of Sedgwick, and thus commenced that long and happy association of two great men, which, though clouded for a time, cannot be said to have been entirely broken up. We may mention here that these volumes are enriched with portraits of some of the chief geologists that have been or are, and nothing more life-like, as far as we know the originals, could be desired.

Another of his early works, in conjunction with Sedgwick, was an account of the structure of the Eastern Alps, which raised much discussion among European geologists, who have not finally accepted the conclusions they contended for—as, for instance, as to the age of the remarkable Gosau beds which they considered to be Tertiary—though they are now generally regarded as Cretaceous.

During all this time he had, like most geologists, avoided as much as possible what he called the "interminable Grauwacké." In the summer of 1831, however, he started with his wife and "two grey nags" to make the first attempt at unravelling the complicated features of these slaty rocks. He determined to begin at the top and trace the succession downwards. In this way he made out satisfactorily that summer the limits and range of the Ludlow rocks. Subsequent summers were devoted to the same work, and arrangements of the Silurian rocks of increasing accuracy were from time to time presented to the Geological Society until his final conclusions made their appearance in the "Silurian System."

On the controversy concerning the nomenclature of the Palæozoic rocks, which led to the painful estrangement between Murchison and Sedgwick, Prof. Geikie throws every possible light, and renders the whole matter perfectly clear. We cannot but think, however, that Sedgwick had more cause for complaint than Prof. Geikie would seem to admit, for if Murchison had no intention to disparage Sedgwick's work, he really, to a great extent, ignored it in comparison with his own. The facts are these. Murchison, in working downwards, described as Lower Silurian the rocks which formed his Caradoc and Llandeilo series, but without defining any satisfactory base line. Sedgwick, in working upwards, described as lying above a series of, at that time, unfossiliferous slates, a set of rocks which he called the Bala group, or Upper Cambrian. Now, though both these geologists went in company over both districts, they failed to discover that these two series were the same—in fact, they pronounced them distinct. Hence, when it was discovered that the one series, the Upper Cambrian, rolled over an anticlinal into the other, the Lower Silurian, each geologist blamed the other for the error. But in the meantime it was ascertained that the fossils were identical, and hence, "zoologically speaking," two different names could not be employed. If, as Murchison supposed, there was a total absence of organic remains beneath these disputed rocks, much might be said in favour of associating them in name with the fossiliferous Silurian rather than with the azoic Cambrian. Yet the manner in which this was done by Murchison, so fully explained by his biographer, leaves little surprise at Sedgwick's indignation, but only that he should have been so long in discovering the drift of what was being done. For in 1842 Murchison writes him a letter, begging the whole question by calling them Lower Silurian, as if there could be no possible idea of calling

them Cambrian, and bidding Sedgwick, if he would retain the latter name, to find some fossiliferous beds *below*. This is followed by the complete dropping out of the name in his "Russia;" and when in after years a series of Lower Fossiliferous beds *were* found, Murchison still sought to include them under the title of Silurian. It is astonishing that Sedgwick should for so long have failed to perceive the drift of these changes—and when he did at length arouse himself he found half his Cambrian system gone, and not unnaturally felt that his friend had "stolen a march on him." Such appears from the data afforded by this work to be the true account of this controversy. In late years, however, chiefly owing to the labours of Mr. Hicks, much new light has been thrown on the succession of faunas in these earliest rocks, and it has been shown that by no means the greatest break in life occurs at the base of the Llandeilo rocks as described by Murchison; and it is therefore probable that the true limits of the two systems will have yet to be re-adjusted under the light of the new facts.

The "Silurian System" is a masterpiece of industry, perseverance, and comprehensiveness, and will be a classical work so long as Geology is a science; it is undoubtedly Murchison's *magnum opus*, and it led directly to those other researches by which he has also contributed so much to our knowledge. Thus it was, on being told that plants had been found in Silurian rocks in Devonshire, that he persuaded Sedgwick to accompany him there, when they found that the so-called Silurians were really of Carboniferous age—but on what did they rest? on a series of rocks with a peculiar assemblage of fossils, which gave them great difficulty at first, but which at last they recognised as a new system, the Devonian, with which they boldly classed the Old Red Sandstone, though no community of fossils had yet been proved. This last step, however, was fully justified, by Murchison's finding in Russia the fishes of the one associated with the shells of the other, and thus the Devonian system was settled on a firm basis.

The received classification, however, of the Devonian rocks was called in question by Prof. Jukes shortly before his lamented death; he assigned the greater part of them to the Lower Carboniferous system, and Prof. Geikie considers it to remain now an open question. He says: "They who have given most attention to this part of geology will probably most readily admit that, whether in the way of contest or not, the question must be reopened; that the accepted classification is far from being satisfactory, and that Jukes did a great service by boldly attacking it, and bringing to bear upon it all his long experience in the south of Ireland, which gave him an advantage possessed at the time by hardly anyone else." Whatever controversy, however, there may be on the classification of particular rocks, there can be no doubt that there is a distinct epoch of life between the Carboniferous and Silurian, and this Murchison and Sedgwick together first defined and established.

It was for the study of the Silurian system, too, that Murchison was led into Russia, and here it was that he found that large development of rocks containing a special fauna overlying the Carboniferous, to which he gave the name of Permian, and which formed the subject of several subsequent researches.

We are greatly indebted to Murchison for the introduction of good names into Geology. It was he who first proposed the use of geographical terms, so happily illustrated in "Silurian," which introduce no theory and no incongruity, such as is involved in calling rocks "transition rocks," or speaking of the Old Red Sandstone as represented by a clay. This method of nomenclature has been widely adopted and is now almost universal, and it has the further advantage of carrying with it information as to the locality where the series is typically developed.

The minor works of Murchison, in the shape of papers and addresses during the time that these "systems" were being worked out, were numerous, and, with the exception of his "Geology of Cheltenham," almost entirely confined to those Palæozoic rocks that had now become so familiar to him. But he brought forward now, not only his own researches, but those of more humble workers also, always giving them due credit. Amongst the most remarkable of these were the discovery of the curious crustaceans of a new type, now known as Eurypteridæ, in the Upper Silurian rocks of Lesmahagow, by Dr. Slimon. Another was the discovery of fossils in the ancient crystalline rocks of the Highlands, by Mr. Peach, which led ultimately to the last of the valuable series of labours that Murchison performed. In the same category as the above must be placed the publication of "Siluria," in which he embodied from time to time, not only his own original researches and additions to them, but the works of all who had laboured in that field, by which the work became at the same time less his own, and more comprehensive than the "Silurian System."

Finally, in the chapter entitled "The Foundatio Stones of Britain," Prof. Geikie gives an account Murchison's last geological work, that of making out the structure of the extreme north-west of Scotland, and discovering there the oldest rocks in Britain. Here, in 1858, he discovered three series of rocks, each overlying the one below unconformably, and it was in the upper of these three that Mr. Peach had found Lower Silurian fossils. If, then, the second be the Cambrian, the lowest must be a series still older. To this he gave the name of Fundamental Gneiss, but afterwards classed it with the Laurentian system of Sir E. Logan, which had been hitherto unrecognised in Britain. This work, however, valuable as it is, is of a different kind to that which made Murchison what he was—a master-builder in Geology.

His chief work consisted in uniting vast masses of rocks stretching over miles of country, variously characterised lithologically, and containing numerous different suites of fossils, into large comprehensive groups; in grasping the features by which many minor periods are united into single systems; in laying down the broad outlines in which the complete geological picture is to be traced. This is the work wanted at the birth of a science; it requires a peculiar power of mind, possessed in large degree by Murchison, who thus deservedly takes rank among the founders of Geology.

We leave Prof. Geikie's work with regret. Like him in writing it, we live again in reading it, with this hero of science; and no one can rise from its perusal without a deeper interest in the progress of knowledge, and especially of geology. A man of great power, thoroughly

devoted to the advancement of science, and pursuing it with energy and discretion, is an example of which we cannot have too many; and the history of Murchison shows how much valuable material may yet be lying dormant in some who have as yet shown no devotion to anything but pleasure and sport.

MARSDEN'S NUMISMATA ORIENTALIA

Marsden's Numismata Orientalia. A New Edition. Part I. "Ancient Indian Weights." By Edward Thomas, F.R.S. (London: Trubner and Co., 1874.)

THIS is the first part of a new edition of "Marsden's Numismata Orientalia," on an enlarged scale, and is the reproduction of an essay published some years ago. As it treats of the earliest information that has come down to us of the system of monetary weights in use amongst ancient Eastern nations, it is considered as an appropriate introduction to subsequent numbers, upon the coins of various Eastern countries, to be contributed by other authors.

Mr. Thomas's essay is a work of considerable interest, not only as regards the information contained in it relating to ancient Indian weights and coins, but also for its philological and ethnological information. The earliest and most important authority cited is from the Sanscrit text of the original code of Hindu law by Manu, the exact date of which is undetermined. Although portions of it are assigned by some authorities between the twelfth and thirteenth centuries B.C., yet the body of the compilation is more generally referred to a period about 400 B.C.

The Indian weights mentioned in the Code of Manu were those of Central India, south of the Himalayas, and comprised between the rivers Indus and Ganges. They were in use after the occupation of this country by the Aryans, whose invasion from the north-west is referred to a period as early as 1600 B.C. Mr. Thomas, however, claims a still earlier origin for this system of ancient Indian weights, and that they were already in use before the Vedic Aryans entered India. The old system appears to have been based on the weight of native seeds. The principal unit was the *Rati*, the seed of the wild liquorice plant. A second unit or standard of weight is stated to have been the *Másha*, a small wild bean, which is also mentioned in the Code of Manu as a food grain. The following tables of monetary weight are taken from the ancient record, and include the smaller seed-grain weights, which, in the original Sanscrit text, are made to originate and lead up to the larger weights in metal, together with the smaller sub-divisions of the seed-grain unit. Their equivalent weight in Troy grains is given by Mr. Thomas as computed from the mean of experimental weighings of the several seeds, and as confirmed from the ascertained weights of less ancient Indian coins.

TABLE I.—*Minor sub-divisions of the Unit, the* Rati.

			Troy grain.
Rati (seed of wild liquorice)			= 1·75
Yava (barley corn husked)		= 1/3 Rati	= 0·5833
Gaura-sarshapa (white mustard seed)	= 1/6 Java	= 1/18 Rati	= 0·0972
Raja-sarshapa (black mustard seed)	= 1/3 Gaura	= 1/54 Rati	= 0·0324
Likhya (small poppy seed)	= 1/3 Raja	= 1/162 Rati	= 0·0108
Trasarenu (mote of sunbeam)	= 1/8 Likhya	= 1/1296 Rati	= 0·00135

TABLE II.—*Multiples of the Unit, the* Rati.

			Troy grain.
Silver.			
Rati.			= 1·75
Mashaka (small wild bean)		= 2 Rati	= 3·5
Dharana Purana	= 16 Mashaka	= 32 Rati	= 56·0
Salamana	= 10 Dharana	= 320 Rati	= 560·0
Gold.			
Masha		= 5 Rati	= 8·75
Suvarna	= 16 Masha	= 80 Rati	= 140·0
Pala, or Nishka	= 4 Suvarna	= 320 Rati	= 560·0
Dharana	= 10 Pala	= 3200 Rati	= 5600·0
Copper.			
Karshapara		= 80 Rati	= 140·0

The fanciful introduction of the "very small mote which may be discerned in a sunbeam passing through a lattice" throws doubt on the practical use of this table; but there appears abundant evidence of the continued use of seed-grain weights in India from a very early period.

The earliest record of Indian measures of capacity, which are only incidentally mentioned in Manu, are quoted from a Sanscrit work for which very high antiquity is claimed. It gives the measures of *ghi*, or clarified butter, in equivalent weights of the *masha* and other multiples of the *rati*.

As to Indian measures of length, though permanently based upon natural units, as the digit, span, and cubit, yet the same seed principle is applied in Manu to the small sub-divisions of the digit. Thus, taking the cubit as the unit, the sub-divisions are stated to have been as follows:—

Hosta (cubit).	
Vitasti (span)	= 1/2 Hosta
Angula (digit)	= 1/12 Vitasti
Yava (very small barley corn)	= 1/8 Angula
Yuka	= 1/8 Yava
Liksha (poppy seed)	= 1/8 Yuka
Balagra (hair's point)	= 1/8 Liksha
Renu	= 1/8 Balagra
Transvarenu (mote of sunbeam)	= 1/8 Renu

The *Hosta*, or cubit, was thus equal to twenty-four digits, or six palms. Mr. Thomas does not assign any particular length to the cubit of *Manu*, but inferentially defines its length from the determined length of the Sikendari *gaz*, or yard, at the end of the fifteenth century, which is rather more than thirty imperial inches. This *gaz* is stated to have been equal to 41·5 digits, and the digit is computed as being equal to 0·72976 inches. This would make the ancient Indian cubit equal to above 17·5 inches.

Mr. Thomas considers that the system of Indian weights here described was indigenous, and he differs from Don V. Queipo, who traces the derivation of the Indian system of weights to primary Egyptian sources. He prefers the "wise reserve of Boeckh," who expresses himself in the following terms:—

"In cases where the weights of measures of different nations are found to be in a precise and definite ratio one to the other—either exactly equal, or exact multiples and parts of each other—we may fairly presume, either that the one has borrowed from each other, or that each has borrowed from some common source. When the ratio is inaccurate or simply approximative, it is to be treated as accidental and undesigned."

The more recent discovery, since the publication of Don V. Queipo's work, of the unit of ancient Egyptian weight, the *Kat* = 140 grains, equivalent in weight to the Indian copper unit, the *Karshapara*, to the gold *Suvarna*, and to one-fourth of the silver *Suvarna*, tends to confirm Don V. Queipo's hypothesis of the identity of the practical units of Egyptian and Indian weights. The Indian